

# SCIENCE

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## THE ISOLATION OF RENNET FROM BACTERIA CULTURES.

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EVER since the beginning of the study of micro-organisms it has been a debated question whether fermentations are to be regarded as biological or purely chemical phenomena. Beginning with the work of Schwann and others in the early part of the century, careful experimentation aided by microscopic study seemed to point to a biological explanation of nearly all forms of fermentation. The work of the third and fourth decades of the century proved beyond question that most fermentations were always intimately associated with the growth of micro-organisms, and the inference was a natural one that the micro-organisms themselves were the cause of the fermentations. Shortly after the valuable work of Schwann, however, appeared the brilliant investigations and discussions of Liebig upon fermentations in general. By Liebig all fermentations were regarded as purely chemical phenomena and the presence of micro-organisms was regarded only as a concomitant incident. According to him all albuminous matter was thought to tend spontaneously toward decomposition, and such decomposition was the basis of fermentation and decay. This, the chemical theory, was for twenty years the favorite theory. The biological and chemical theories were very rigidly opposed to each other and supposed to be contradictory. With the work of Pasteur and the great development of the study of micro-organisms thereby inaugurated, the biological theories of fermentations again came to the front, rapidly gained the ascendancy, and soon displaced almost entirely the chemical theory as advanced by Liebig. For the next twenty years it was regarded as an almost settled fact that most natural fermentations were biological phenomena, and the theory of Liebig was at last practically abandoned.

Within still more recent times there has been a partial swinging back of the pendulum toward a chemical explanation of many forms of fermentation at least. This has not, however, been in the direction of Liebig's theory, but rather toward a theory of the action which unites together a chemical and biological explanation. It has been recognized for more than half a century that there are forms of ferments, such as pepsin, trypsin, etc., which do act in a purely chemical manner. These ferments, it is true, are produced originally by living organisms, but when once produced they are not living themselves, at least in any proper sense, and their action is not dependent upon growth or multiplication, for they are not organisms. So far as can be determined their action is purely chemical. On the other hand, a large number of fermentations, such as the alcoholic fermentations, the souring of milk, etc., have been traced with certainty, not only to living organisms in the form of bacteria and yeasts, but to the actual growth and multiplication of these organisms. These fermentations occur only when micro-organisms are present and only when these micro-organisms grow and multiply. The amount of growth of the organism is a measure of the amount of fermentation. Undoubtedly these biological fermentations are of a different nature from the other class. We are, however, learning now to look upon some of the biological fermentations as chemical in their immediate nature.

In the first place, the bacteriologist has been learning that germ diseases, which are caused primarily by the growth of micro-organisms in the body, are caused immediately by certain poisonous bodies which these organisms produce. He has called

these bodies ptomaines, and at present biologists are very rapidly becoming convinced that it is the direct action of these poisonous materials which produces the symptoms and disturbances associated with most germ diseases. It is not the simple growth of bacteria which produces disease, but the poisonous products of their growth; and thus a chemical explanation is added to the biological.

Not only in germ diseases but in other forms of fermentations, not associated with disease, bacteriologists are learning of the production of chemical ferments by the micro-organisms. Many organisms have been found to produce diastase, sucrase, glucase, etc. Within the last year or two it has been demonstrated that many bacteria produce a chemical ferment very similar in its character to trypsin. The general class of bacteria which liquefy gelatin have long been known to have an action quite similar to that of pancreatic juice. Indeed, it is this peptonizing action which is the cause of the liquefaction of the gelatin. Recently Brunton and Macfadyen and especially Fremi have succeeded in actually isolating from bacteria cultures a chemical ferment which has this power even when acting in sterilized media.

Some recent work in the bacteriological laboratory at Wesleyan University has isolated another chemical ferment from bacteria cultures. A large class of bacteria have the following actions on milk: They first curdle the milk rendering it slightly alkaline; subsequently the curd is slowly dissolved into a more or less watery liquid. Chemical study shows that this last action is a simple digestion and peptonization of the curd and that it is due to the trypsin-like ferment above mentioned. The curdling, which precedes the digestion, however, must be due to a different action, inasmuch as trypsin produces no curdling of the milk. It has been suspected for some time that this curdling is really due to a "rennet-like" ferment which is produced by the bacteria. It has been my good fortune recently to demonstrate the truth of this supposition. My method of work has been as follows:—

The bacteria in question are cultivated in milk for several days, in some cases for two weeks. By this time the curd is precipitated and at least partially dissolved, and the result is a somewhat thick liquid containing, of course, immense numbers of bacteria. This liquid is filtered through a porcelain filter to remove organisms, and a clear, usually amber-colored, filtrate is thus obtained. The filtrate, of course, contains in solution all of the soluble chemical ferments which may have been formed by the bacteria. This filtrate is now acidified with  $\text{H}_2\text{SO}_4$  and then common salt is added to a state of super-saturation. When this condition is reached there appears on the surface of the liquid a considerable quantity of snow-white scum. This scum is removed from the liquid, purified if necessary by reprecipitation, and then dried. It produces a snow-white powder, which upon experiment is found to be active in its curdling action upon milk and to have all of the essential characters of rennet. The ferment which is thus obtained is not chemically pure, containing, besides the rennet ferment, a varying amount of the tryptic ferment formed at the same time. But the rennet ferment is most abundant and is very active. This ferment can be kept indefinitely, is killed by heat, acts best at a temperature of  $30^\circ\text{--}35^\circ\text{C.}$ , and curdles sterilized milk under proper conditions in half an hour. Experiment shows that no organisms are present in the curdled milk, and there is thus no doubt left that we are dealing with a chemical ferment similar to rennet, and which is produced by the growth of these micro-organisms in milk. The ferment does not appear to be exactly identical with rennet, some of its chemical tests being different. This may be due to the impurities which are present or to an actual difference in the ferment.

A large number of bacteria possess this power of producing rennet, though not more than nine or ten have thus far been experimented upon. Those studied differ much in the amount of the ferment produced, some giving large quantities, and others only traces. Thus far it seems that all species of bacteria which liquefy gelatin produce this rennet ferment, although some of them only in small amount.

This general line of work is thus leading bacteriologists to a better understanding of the fermentations, although we are as yet doubtless far from any real knowledge of their nature.

# IS THE SÃO FRANCISCO DO SUL (SANTA CATHARINA) IRON A METEORITE?

BY ORVILLE A. DERBY, SÃO PAULO, BRAZIL.

THE possibility of a terrestrial origin for masses of native iron being established by the well-known occurrence at Ovifak, Greenland, doubt may be cast on the mode of origin of such so-called meteorites as in their chemical and physical characters depart widely from the ordinary type of meteoric iron. From its size and prominence in meteoric literature the most important example of such a doubtful iron is that found in 1875 near the city of São Francisco do Sul, in the State of Santa Catharina, Brazil, and generally known as the Santa Catharina iron or meteorite. Professors Daubree and Stanislas Meunier regard it as undoubtedly meteoric; while Dr. A. Brezina, whose opinion is equally entitled to respect, considers it as probably terrestrial, placing it alongside of the Ovifak iron in his catalogue of the Vienna collection.

So far as known to the writer, no such minute study as Brezina, Cohen, and others have given to various undoubted meteorites has as yet been made of the Ovifak iron, so that at present we are in the dark as to whether or not it presents definite criteria by which a terrestrial iron can be positively separated from a meteoric one. So many of the characteristics once presumed to be purely meteoric have already been noticed, either in the Ovifak or artificial irons, that it may be doubted if any such definite criteria exist. The published analyses of Ovifak show that neither the nickel-iron alloys nor the association with a monosulphide (troilite) carbon free or combined and phosphorus can be taken as distinctive of meteorites. It is not clear, however, if the carbon presents the same form as in meteorites, that is to say, free in the form of amorphous carbon, graphite, cliftonite, diamond (?), etc., and combined as cohenite, nor if the phosphorus is combined with iron and nickel as in the meteoric minerals schreibersite and rhodite. On the other hand, the compact (*Dichte Eisen*) group of Brezina containing at least nine undoubted meteorites, one of which, Nedagolla, was seen to fall, proves that the absence of a certain crystalline structure, indicated by the so-called Widmanstätten and Neumann figures, is not necessarily proof of non-meteoritic origin. On the contrary, evidence is accumulating that a very similar if not absolutely identical crystalline structure may, under favorable circumstances, appear in artificial irons. Huntington has illustrated figures very like those of Widmanstätten in *spiegel-eisen*; Linck has described a crystal from a furnace slag, with cube faces and a polysynthetic twinning structure, which he identifies with the Neumann figures, and a perfect octahedral crystal with a similar twinning structure has recently come into the possession of the writer from a Brazilian blast-furnace.

It may be presumed that it is mainly on the absence of the characteristic meteoric figures that Brezina depends in placing the São Francisco do Sul with the Ovifak iron, as in no other respect are they markedly similar. The Brazilian iron does, however, show, at least in places, a very fine rectilinear cross-hatching indicative of polysynthetic twinning, but according to some law different from that giving the Neumann lines. Another point of agreement is in the abundant occurrence of the magnetic oxide of iron which is either absent from most meteorites or has been overlooked in their description. Both irons are brittle, being readily broken to fragments with a hammer, in which re-

spect they differ from the tenacious malleable metal of most meteorites. Both also crumble to fragments in the atmosphere of museums, though apparently not from the same cause, the crumbling of São Francisco do Sul being due to alteration of the sulphide, which presumably is not the case with Ovifak.

Lawrence Smith and Becherel found the magnetism of the Brazilian metal abnormally weak but that it became normal on heating, from which they concluded that the mass could not have been subjected to great heat. There seems, however, to be a variation in this respect in different parts, since Daubree notes that many fragments exhibit polar magnetism while others do not. Any argument drawn from the magnetic properties would apparently tell as much against a terrestrial as a cosmic origin, since the only conceivable mode of terrestrial origin is in the midst of an igneous magma made fluid by heat.

In other respects, however, a parallel for the characteristic features of the São Francisco do Sul iron is to be looked for in the group of meteoric irons rather than in those, so far as published, of the terrestrial iron of Ovifak. It is particularly characterized by the high percentage of nickel and the extraordinary abundance of sulphide. In the first respect its nearest ally is the Oktibbeha meteorite with nearly double the proportion of nickel, and not the Ovifak iron in which that metal is below the meteoric average. Unlike any other known iron, meteoric or terrestrial, the sulphide forms a gangue inclosing the metallic parts, but it is interesting to notice that it also presents itself in pencil-like inclusions in the metal, surrounded by carbon and other accessories, as in the Bendegó meteorite. The meteoric phosphurets, schreibersite and rhodite, not yet described from, though perhaps present in, the Ovifak iron, are abundant accessories. In the carbonaceous residue, soft, friable granules, with a crystalline form suggestive of the cliftonite of the Youngedin, Magura, and Bendegó meteorites, have been noticed.

Thus, so far as at present known, the chemical and physical characteristics of the São Francisco do Sul iron do not point very markedly to an association with that of Ovifak. What is known of the geological conditions of the place of discovery, although too incomplete to be decisive, points rather to a meteoric origin.

The geological information regarding the place of discovery is derived from verbal communications by the late Professor Ch. Fred. Hart, a paper in the *Revista do Observatorio* de Rio de Janeiro for 1888 by Dr. Luiz F. Gonzaga de Campos, and a recent article in the *Jornal do Commercio* of Rio, May 29, 1892, by Dr. J. P. Calogeras. All these accounts agree in representing the island of São Francisco do Sul as composed essentially of granitoid gneiss cut by dykes of tourmaline granite and diabase and covered by a heavily-wooded soil-cap due to the decay of the underlying rocks. That is to say, it is a typical locality of the coast-belt from Cape Frio to Montevideo, at any point of which, so far as geological indications go, native iron might be looked for with as great probability as at this particular locality. The rocks, granite, gneiss, and diabase, are well exposed about the shores of the island. The latter, which approaches most nearly in character the Ovifak rock, being apparently no more abundant than in any other similar locality. In the interior of the island the soil-cap and forest growth make geological observations difficult. Dr. Campos, in his examination of the place of discovery of the iron, opened numerous paths and pits, and on my recommendation paid particular attention to the question of the possible occurrence of basic rocks in immediate connection with the iron. He says, "Although the rock on this hillside is much altered, giving an argillaceous soil of a red color, here more, there less deep in tint, it shows perfectly in some points the mineral composition of granite." At the top of the hill near the point *c* [one of the points where masses of iron were found] there are large blocks of granite, at times tourmaliniferous. In the vicinity, in all the directions that I followed, all the soil is granitic. In the bed of the stream, which I ascended in various sections, the material was always that of the disaggregation of granitoid gneiss. Finally, I did not find at this place a single exposure of basic rock." The numerous specimens that have come to hand showing foreign material cemented to the iron by the limonite crust formed by its alteration are in accord with this description, indi-

cating, as Daubree has always remarked, that the iron rested on a granitic soil.

The topographical sketch of the locality made by Dr. Campos shows no indication of concealed dykes of basic rocks, which, according to universal experience of occurrences under similar conditions in Brazil, should, from their more ready decomposition, form depressions on the surface. The principal mass was found at the foot of the hill near a creek on the point of a spur, and aligned with a mass about 200 meters distant at the extreme summit of the hill and with an intermediate find close to an exposure of granite on the same spur. The other finds are aligned parallel to the course of the creek. In one pit on this line iron was found to a depth of 2.8 meters, covered with wash earth brought down by the rain from the higher portions of the hill.

Dr. Calogeras, who argues in favor of a terrestrial origin, presents no facts in disaccord with the above observation that the only rock known in the immediate vicinity of the iron is granitic. His argument, based on the occurrence in the region (principally at a distance of several miles) of iron and manganese ores (oxides), and presumed to be connected with dykes of diabase, and of a small percentage of nickel (0.30 per cent) in one of his specimens of an argillaceous manganese ore, has no direct bearing on the question of origin. Even if the native iron had been found resting upon the said ore deposits instead of several miles away, a genetic relation would still have to be proven, and until direct evidence was presented most petrologists would probably regard the relation as casual rather than genetic.

No specimens of the diabase of the vicinity of São Francisco have come to hand. Assuming that it is of the usual character of the diabases of similar regions in Brazil, it is not so unlike the basalt, or dolerite, of Ovifak that a comparison might not with propriety be made. It neither approaches nor differs from the iron-bearing Greenland rock more than do the normal diabases of other parts of the world, in which as yet nothing analogous with the Ovifak occurrence has been noted. If the diabase or some related highly basic rock had been found in immediate contact with the iron, a comparison with the Ovifak occurrence would be justified, but even then complete proof of a terrestrial origin would be lacking. As the case stands at present, with tolerably satisfactory evidence that the iron rested on highly acid rocks or their debris, the hypothesis of such an origin involves that of the formation of native iron under conditions entirely different from those of Ovifak.

Another argument in favor of the meteoric origin of the iron may be drawn from the state of preservation of the masses. Although the metal itself is more than usually resistant to oxidizing agencies and to the action of acids, the abundant presence of pyrite renders it peculiarly susceptible to alteration. All the smaller masses are completely changed to limonite, pieces of the size of a man's head or larger are in large part altered, but still show remnants of metal badly fragmented and oxidized in the centre, and only the larger masses retain perfectly sound metal and sulphide. Even in the dryer air of museums it is not a good-keeping iron, the disintegrating action of the decomposing sulphide being singularly favored by the fragmented condition of the metal. Under these circumstances it is extremely improbable that, buried in the soil and exposed to the extremely rainy climate of the coast region of southern Brazil, the iron could have been preserved for more than a few centuries at the utmost. The hypothesis of a terrestrial origin involves the exposure to destructive agencies through untold geological ages, since the present topographical features of this part of the Brazilian coast are unquestionably extremely old. In view of the Ovifak occurrence, it is possible and even probable that native iron of terrestrial origin simulating meteorites will be found in other parts of the world and perhaps in rocks of different petrographical types. It is not probable, however, that the first discoveries of this character will be made in surface exposures in the extremely humid coast region of southern Brazil, where the country rock is of Archean age, and the eruptives presumably date back to the beginning of the Secondary age, if not earlier.

## NEW TREATMENT FOR SNAKE-BITE AND OTHER POISONS.

BY W. H. WOOSTER, PRESIDENT OF THE BALLARAT ASTRO-PHYSICAL SOCIETY, VICTORIA, AUSTRALIA.

AS poisonous snakes are more or less common in many countries, and the circulation of *Science* is world-wide, and other cases of poisoning often occur, and as I have been the means of saving a life by a new process, one that can be applied when it is too late for the orthodox method of cutting and sucking, and used by anybody, with materials at one's hand in every house, I have concluded that I should not be doing my duty if I did not make it known. Some time since, when living in the country, one of the nicest little girls of my acquaintance, about four years of age, was brought to me by an elder sister for diagnosis and treatment. She was swelling from head to foot, becoming cold and stiff in the limbs, and losing her power to answer or even understand questions. As I had been the means of effecting several simple cures in the district, she was sent in the hope that I would be able to tell instinctively what was amiss, and to cure it as if by magic. As the sequel proved, the latter was almost realized, notwithstanding that in regard to the former I was quite at sea. She had never known what a snake was, but for strategic purposes, well-known to managers of children, had often been terrified with the name of "bulldog" without knowing what that was (bulldog was the popular name for a very poisonous, pugnacious, and gigantic ant, *Myrmecia vindex*); so that whenever she got stung or bitten by anything, it was put to the credit of the bulldogs, as on this occasion. She had screamed and fallen a few yards from the house, and told her mother a bulldog had bitten her on the foot; and that was all she knew. The foot was examined, but from running barefoot was so full of scratches and punctures that none could be fixed on as certainly the marks of snake-fangs. The mischief had occurred about an hour before I saw her, and while being examined she was getting rapidly worse; the swelling, coldness, and stiffness were becoming alarming, the lips as thick as one's thumb, the hollows on each side of the nose were filled up level, and of a steel-blue and sea-green color, the arms, lower limbs, and body were becoming blotched with irregular raised parts, white and hard, the spaces between being sunk and dark-purple; the pulse, too, was getting exceedingly feeble. Not thinking a bulldog ant could produce such effects, and not being certain that it was a snake-bite, I concluded that it might be a spider-bite, as my only brother had nearly lost his life from that cause. Even if the place of the bite or sting could have been found, it was clearly too late to cut and suck, for the poison was already all over the body, and rapidly mastering the vital functions; besides which, no one in the district had an ammonia syringe for hypodermic injection. The question was, What could be done? Precedent said: Send for a doctor. But there was none nearer than eight miles, and then he might not be at home; or, if at home, most likely intoxicated; and, besides that, she looked as though she would die before a doctor could see her.

In this conflict of thought and feeling, a happy idea struck me. I had proved in my own person the power of a hydropathic, hot-sweating-pack to produce a flood of perspiration, and throw off impurities from the blood, and it now occurred to me that if I could sweat the poison out from the whole surface, it would not matter where it got in, nor what put it there; and, moreover, that if it were any good, the danger would be over before anyone could get half-way to the doctor's; and, if twenty minutes or so produced no benefit, the doctor could still be sent for as a last resource. It was a great responsibility, but under the circumstances I felt it a duty, and went to work. Of course, there was no hot water ready, but we soon made some, and put it into a tub, into which the child was placed, with a blanket over all, tucked in close round the neck to keep the steam in, but leaving the head out. This was to open the pores of the skin quickly. While in this I spread a piece of oilcloth on the table, and a pair of blankets on that. As soon as more hot water was ready, a sheet was wrung tightly out of it, and spread on the blankets. The child was laid on this, and then first one side, and then the other lapped over her, and it was tucked in close about the neck; then the

blankets followed, and lastly the oilcloth, and she was put to bed, with another pile of blankets on top. Then some spirits were got to keep the heart-action up, which by this time had almost ceased. Before this the mother felt sure the child was dying, and was nearly frantic with the idea. Hot brandy and water was given in a teaspoon every few minutes, and the case was watched with no little anxiety. She had not been in the pack over fifteen minutes before improvement became apparent. The dark rings round the eyes were less marked, the eyes themselves brighter and less sunk, and the blue and green tints less ghastly. Our hopes began to revive, and our fears to lose their terror. In five minutes more the improvement became so decided that with great gratitude I felt that the novel plan was a grand success, and the danger over. She now became conscious, and, evidently feeling the benefit of the spirit stimulus, asked occasionally for her "toddy," which she, knowing as much about it as she did of snakes and bulldogs, called "vinegar and milk." As the need and benefit of it grew less, she liked it less, and finally refused it. After something over an hour, we took her out of the pack, and were delighted to see that all the swelling, blotches, stiffness, and discoloration had completely disappeared, and Amy was herself again. She was now washed down in cool water, to close the pores and prevent catching cold, and put to bed as usual. She was left with strict injunctions that I should be called up if anything went wrong during the night, but my sleep was not disturbed. Next morning I went to see my little patient, and found her at the breakfast table, with as good an appetite as ever. After that we can excuse the mother for thinking that the hot sweating-pack was the panacea for "all the ills that flesh is heir to."

But some will ask, Why call this a case of snake-bite? When she recovered, we questioned her as to the size and appearance of the "bulldog," and she described it as "a big, long, pretty thing." When asked how many legs it had, she said, "No legs; a big pretty thing, as long as my arm, all shiny." But evidence still more definite was at hand. A few days after, the father, who was up-country at the time of the occurrence, sank a well near where she had fallen, and where there was a lot of long grass and loose timber, and, having struck water, stopped for a rest and a cup of tea. When he returned, a black snake (*Pseudechys porphyraicus*), having smelt the water, was down in the well. He came to tell me that he thought he had caught Amy's bulldog. Then we took her to the well without telling her anything of what was in it, and asked her if she had ever seen a thing like that; and directly she saw it she said, "Yes, that's the bulldog that bit me."

Of course the ligature-cut-and-suck method is best when applied in time, and when the bitten spot is known; but it would be utterly useless in such a case as this, where the poison had already been carried all over the body. The method here advocated would be applicable, I believe, to almost all cases of poisoning that had reached the same stage, whether from snakes, spiders, scorpions, insects, and such like, or from poisons taken by the mouth, whether drunk as liquids or eaten as poisonous fish, etc.; and I have no doubt would save many a valuable life after the venom had got too far through the system for local sucking, or even the stomach-pump, to be of any avail.

#### ON THE INTRODUCTION OF THE EUROPEAN BARK-BEETLE-DESTROYER (CLERUS FORMICARIUS L.) TO AMERICA.

BY CAMILLO F. SCHAUFUSS, DIRECTOR, MUSEUM LUDWIG SALVATOR, MÜNCHEN, GERMANY.

DIE forstlichen Verhältnisse Europas und Amerikas sind durchaus verschieden:

In dem alten Europa — wir haben hier also die älteren Culturstaaten, namentlich Mitteleuropa, im Auge — finden wir eine Jahrhunderte alte, rationelle Forstbewirtschaftung durch akademisch gebildete, fachkundige Männer, infolgedessen, was uns als erste Vorbedingung für einen *geordneten* Waldbau erscheint, ein vorzügliches Strassenwesen innerhalb der Forsten. Damit ist ein leichter und billiger Transport der geschlagenen Bäume vorhanden.

In dem jungen Amerika haben wir noch grosse Strecken ganz jungfräulichen Gebietes, sonst grossentheils eine verhältnissmässig junge, ja, wohl *sehr* junge Forstwirtschaft; infolgedessen ein noch ungenügend entwickeltes oder unentwickeltes Strassenwesen innerhalb der Forsten, und damit eine schwere und theure Abfuhr der Hölzer.

In Europa ferner: eine dichte Bevölkerung, mithin ein flotter Absatz für das Holz, Rinden und Zweige *an Ort und Stelle*.

In Amerika: eine dünnbesäte Bevölkerung, also Mangel an Absatz, langer und kostspieliger Transport der Baumstämme bis zum Verkaufsplatze, während die Wipfel und Zweige der Bäume wegen der ungenügenden Wege und theuren Fortschaffung meist im Walde liegengelassen werden müssen.

Es ist unter diesen Umständen gar nicht möglich, gegen auftretende Waldverwüster, wie solche die Insektenwelt in so grosser Zahl stellt; mit den, beispielsweise in dem forstlich hochentwickelten Deutschland üblichen, *radicalen* Vertilgungsmaassregeln vorzugehen, auf welche einen Blick zu werfen wir uns für einen späteren Aufsatz vorbehalten.

Nun hat die Natur fürsorglich gar wohl darauf gesehen, dass in ihrem Haushalte das Gleichgewicht erhalten bleibe; sie hat deshalb auch dem Ueberhandnehmen der einzelnen Thiere ein Ziel gesetzt, indem sie ihnen Feinde zugesellte. So haben besonders die Insekten, ausser den Vögeln, sehr viele Nachsteller unter ihresgleichen. Da ist die grosse Zahl der Raubinsekten aller Ordnungen, welche als Strassenräuber über alle Kerfe herfallen; dann giebt es besondere Feinschmecker, die sich nur an eine Fleischsorte halten, deshalb allenthalben mit ihrem Nahrungsthier zusammen vorkommen; und endlich die heimtückischen Schleicher, die entozoischen Parasiten, welche in so grosser Zahl unter den Hymenopteren und Dipteren sich finden.

Bei der Natur ging der Forstmann in die Lehre. Ratzeburg,<sup>1</sup> der grosse bahnbrechende deutsche Forstentomologe, berichtet von 1868 in Posen (Preussen) vorgenommenen Versuchen mit Uebertragung von Maulwürfen auf von Engerlingen (Larve des Maikäfers) bedrohte Kulturflächen, Versuchen, die nicht ohne Erfolg blieben. Weiter hat derselbe Gelehrte die Waldameise (*Formica rufa*)<sup>2</sup>, sowie Schlupfwespen nach von Schmetterlingsraupen heimgesuchten Gegenden übertragen, und andere Fachmänner sind seinem Beispiele gefolgt. In neuester Zeit hat namentlich C. V. Riley, soviel ich gehört habe, wiederholt Experimente mit der Translocation von Schlupfwespen gemacht.

Wie im July dieses Jahres Mr. Andrew D. Hopkins von der West Virginia Agricultural Experiment Station in Morgantown den Lesern unseres Blattes mittheilte<sup>3</sup>, ist in den letzten Jahren in West Virginia der Borkenkäfer, *Dendroctonus frontalis* Zimm., in solchen Unmassen aufgetreten, dass auf einem Raume von 10,000 square miles 75% aller Nadelbäume kranken oder abstarben.<sup>4</sup> Der genannte Staat gehört aber zu denjenigen, wo eine rationelle Waldkultur, mithin eine rationelle Vertilgung des Borkenkäfers noch nicht möglich ist. Mr. Hopkins fasste deshalb den Gedanken, um wenigstens zu thun, was möglich ist, um dem Waldfeinde entgegenzutreten, nachdem er beobachtet hatte, wie *Clerus dubius* F. sich alle Mühe gab, mit den Scolytiden aufzuräumen, ihm einen Helfer in seinem guten Werke zur Seite zu stellen.

Europa und Nordamerika haben seit langer Zeit, wie dies der rege Verkehr zwischen beiden Erdtheilen und die Gleichartigkeit des Klimas mit sich bringt, gegenseitig ihre Schädlinge ausgetauscht: ich erinnere an den Kartoffelkäfer (*Leptinotarsa decimlineata* Say) in Deutschland und an den Kohlweissling

<sup>1</sup> Ratzeburg, Die Waldverderber, 1841, pp. 21, 22.

<sup>2</sup> Ratzeburg, Die Waldverderbniss, 1868, II., p. 429.

<sup>3</sup> Science, Vol. XX., No. 495.

<sup>4</sup> Dies wird erklärlich, wenn man die beträchtliche Fruchtbarkeit der Scolytiden kennt. In der Mitteleuropäischen Forstinsektenkunde stellen hierüber Judeich und Nitzsche folgendes Rechenexempel auf: Nehmen wir an, ein Mitte April fliegendes Weibchen habe in seinem Muttergange 90 Eier abgelegt, so können wir mit Sicherheit darauf rechnen, dass im Anfang Juni wenigstens 30 Stück davon zu fortpflanzungsfähigen und wirklich begatteten Weibchen sich entwickeln. Legt jedes dieser 30 Weibchen wieder einen Muttergang mit 90 Eiern an, so produciren sie also zusammen 2700 Stück, und wird Anfang August beim dritten Fluge wieder nur ein Drittel davon zu Weibchen, so nagen diese schon 900 Muttergänge und belegen sie mit 8100 Eiern. Gelangt von diesen wieder nur ein Drittel im nächsten Frühjahr zum Eierlegen, so kommen im April bereits 27,000 Nachkommen des *einen* im vorhergehenden April geflogenen Weibchens zur Fortpflanzung und können nun 2,430,000 Eier ablegen!

(*Pieris brassicae* L.) in den Vereinigten Staaten. Warum also nicht auch die Nützlinge?

Mr. A. D. Hopkins that wohl sehr gut, sein Augenmerk auf Europa zu richten.

Und in der That haben wir in *Clerus formicarius* L. einen Käfer, der nicht nur in seinem Aeusseren, in Grösse und Färbung, seinem amerikanischen Bruder, *Clerus dubius* F., überaus ähnelt, sondern dem Letzteren auch in seinen Lebensgewohnheiten gleicht. Er ist, sowohl als Larve, wie als Imago, ein scharfer Feind der Nadelholz-Borkenkäfer, gleichviel ob sie auf Kiefer oder Tanne leben, und dabei recht zahlreich.

Diesen wählte Mr. Hopkins zur Einführung nach Amerika.

Um sein Ziel sicher zu erreichen, setzte er sich mit dem durch seine klassische Monographie berühmten Scolytiden-Specialisten Eichhoff und mit mir in Verbindung und kam im August nach Europa, hier die Lebensbedingungen des Thieres zu studiren und zu sammeln.

Mr. Eichhoff schrieb mir, während Herr Hopkins im Elsass sammelte, dass derselbe "mit seltenem Geschick und grossem Glück" arbeite, und ich selbst konnte dies sehr bald aus eigener Anschauung bestätigen, als ich mit Mr. Hopkins gemeinschaftlich mehrere Tage in den sächsischen Wäldern auf Cleriden fahndete. Wir fanden die Larve in allen Stadien des Wachstumes, die Puppe, die eben entschlüpfte Imago, und den kräftigen, lebhaften Käfer in ihrem kunstvollen Winterquartieren innerhalb der Rinde.

So kann denn Mr. Hopkins zufrieden auf den Erfolg seiner Reise blicken, denn er nahm eine stattliche Zahl Cleriden in allen Stadien, der Sicherheit halber in verschiedener Weise verpackt, mit nach Hause. Und da es wohl keinem Zweifel unterliegt, dass der weitaus grössere Theil der in der Winterruhe befindlichen Thiere gesund ankommt, — wenn nicht übertriebene Cholerafurcht etwa Herrn Hopkins Schätze durch Disinfection verdirbt, — so kann im Frühjahr mit dem Acclimatisationsversuche begonnen werden.

Für genügenden Nachschub wird von mir eventuell gesorgt werden, um Mr. Hopkins's Experiment gelingen zu lassen.

#### COLIAS EDUSA AND COLIAS HYALE.

BY A. HEATH, LONDON, ENGLAND.

GREAT BRITAIN this year has been favored with an abundance of these beautiful insects; from every part come reports of innumerable captures, especially of *C. Edusa*, many insects being taken at one throw of the net. *C. Hyale* has also been, I may say, plentiful when we consider its comparative rarity here; friends of mine report taking during a few days as many as four or six this season. I have myself taken four fine specimens in as many days. The first specimen I took in June, but it was the only example of *Colias* that I saw until August; as a rule these insects are never taken in England until August. In 1886 we had a similar year; enormous numbers of *C. edusa* were to be seen, one entomological friend told me he had seen a certain field in Kent yellow with them. It seems most extraordinary that this year we should have had such an abundance of this particular insect, when last year scarcely one was to be seen even in their favorite localities.

Some entomologists believe that they come across the English Channel (over 20 miles of water) in swarms, but if this were the case, surely someone would see them arrive or on their arrival before they scattered over the country. Then, if this were so, why do we not get an annual visit in quantity? The insect is always in abundance on the continent of Europe, and there is also an abundance of many other kinds of butterflies that we seldom or never see here. My idea respecting these occasional abundant swarms is that butterflies' eggs are indestructible, and will lie on the ground for years until a favorable season arrives.

The eggs of *Colias* are laid on the food-plant, various forms of *Trifolium*; this is not only their food, but the food of every kind of four-footed animal, domestic and otherwise, inhabiting this country (except carnivora), and the whole field or crop of *Trifolium* is eaten either in a green or dry state. What, then, be-

comes of the eggs deposited? They must be eaten up almost entirely, and if not indestructible they would be destroyed. This seems not to be the case, and it is probable that they can pass through the animal uninjured by the heat of its body, and so be again distributed over the ground. Without some such theory it seems almost impossible to account for the large numbers found in a cultivated country following a year of scarcity like last year, especially when we remember the enormous number of larvæ destroyed by ichneumon and other countless enemies, bad seasons, etc.

#### NOTES AND NEWS.

THE New York Academy of Sciences has recently organized a biological section which will hold monthly meetings. At the opening meeting, Oct. 17, Professor Henry F. Osborn acted as chairman. The following papers were presented. Bashford Dean, "On *Dionæa* under its Native Conditions near Wilmington, N.C.," the results of experiments emphasizing the plant's erratic sensibility and its special adaptability for capturing ground insects; N. L. Britton, "On a species of *Hieracium*;" E. B. Wilson, "On the Artificial Production of Twins and Multiple Embryos in *Amphioxus*." The paper dealt mainly with the peculiarities of double monsters produced (as in Driesch's experiments on *Echinus*) by shaking apart the blastomeres of two- and four-celled stages (v. *Anatomischer Anzeiger*, 1892). Every gradation exists between two perfect and separate bodies, each half the normal size, and four in which the only indication of duality consists of a bilobed condition of the archenteron. In the double gastrulas the long axes of the two halves may form any angle with each other, and the two blastopores when separate may be turned in any direction. In cases where the two blastopores face each other, the two bodies are united by a bridge of tissue at one side, essentially as in the double gastrulas of certain earthworms.

— As cotton-seed meal is gradually coming into use in Ohio as a valuable adjunct to the ration for dairy cows, and as the scarcity and consequent high price of corn the present season may tempt some farmers to add this meal to the pig ration, it seems advisable to call attention to bulletin 21 of the Texas experiment station (located at the Agricultural and Mechanical College of Texas, College Station P. O.). In this bulletin Director G. D. Curtis reports the results of a long series of experiments in feeding cotton-seed to pigs, from which he comes to the conclusion that there is no profit whatever in feeding cotton-seed in any form to pigs, whether the seed be boiled, roasted, or ground. The ground seed seems to have produced the worst results, causing the death within six to eight weeks of a large proportion of the pigs to which it was fed, and especially of the medium and small-sized shoats. The boiled seed was less injurious, but roasted seed was almost as fatal as the meal. These pigs were fed alongside of similar pigs which had corn instead of cotton-seed, and the corn-fed pigs remained in perfect health. The symptoms produced by the cotton-seed are described as follows: The first sign of sickness, appearing in from six to eight weeks after cotton-seed meal is added to the ration, is a moping dullness of the animal, with loss of appetite and tendency to lie apart. Within the course of twelve to thirty-six hours, often within the shorter time, the animal becomes restless; staggering in his gait; breathing labored and spasmodic; bare skin showing reddish inflammation; sight defective, and both the nervous and the muscular systems feeble and abnormal in action. The fatal cases all show "thumps" — spasmodic breathing, and in many instances the animal will turn in one direction only, following a fence, or building wall, so closely as to strike his nose against projections in a vain endeavor to push outward in that one direction which he tries to take. If no fence or building intercept him he may travel in a circle — large or small according to the mildness or acuteness of the malady in his particular case. When exhausted by his efforts the animal drops down suddenly — sometimes flat upon his belly, sometimes dropping on his haunches with his fore legs well apart to keep from falling over — almost always with the evidence of more or less acute internal pain. At death a quantity of bloody foam exudes from mouth and nostrils.



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## WHY IS SEA-WATER SALT?

BY W. MATTIEU WILLIAMS, F.R.A.S., F.C.S., LONDON, ENGLAND.

THIS question has been regarded as a mystery and has given rise to some curious speculations, but a little consideration of the subject must, I think, satisfy us all that it would be very wonderful, quite incomprehensible, if the waters of the ocean were otherwise than salt as they are.

The following explanation was first suggested to myself many years ago when receiving my first lessons in practical chemical analysis. The problem then to be solved was the separation of the bases dissolved in water by precipitating them, one by one, in a solid condition; filtering away the water from the first, then from this filtrate precipitating the second, and so on, until all were separated or accounted for.

But in doing this there was one base that was always left to the last, on account of the difficulty of combining it with any acid that would form a solid compound, a difficulty so great that its presence was determined by a different method. This base is soda, the predominating base of sea-salt, where it is combined with hydrochloric acid. Not only is soda the most soluble of all the mineral bases, but the mineral acid with which it is combined forms a remarkably soluble series of salts, the chlorides. Thus the primary fact concerning the salinity of sea-water is that it has selected from among the stable chemical elements the two which form the most soluble compounds. Among the earthy bases is one which is exceptionally soluble,—that is, magnesia,—and this stands next to soda in its abundance in sea-water.

Modern research has shown that the ocean contains in solution nearly every element that exists upon the earth, and that these elements exist in the water in proportions nearly corresponding to the mean solubility of their various compounds. Thus gold and silver and most of the other heavy metals are found to exist there. Sonnenstadt found about 14 grains of gold to the ton of sea-water, or a dollar's worth in less than two tons.

As the ocean covers all the lower valleys of the earth, it receives all the drainage from the whole of the exposed land. This drainage is the rain-water that has fallen upon this exposed surface, has flowed down its superficial slopes, or has sunk into porous land, and descended under-ground. In either case the water must dissolve and carry with it any soluble matter that it meets, the quantity of solid matter which is thus appropriated being proportionate to its solubility and the extent of its exposure to the solvent. Rain when it falls upon the earth is distilled water nearly pure (its small impurities being what it obtains from the air), but river-water when it reaches the ocean contains measura-

ble quantities of dissolved mineral and vegetable matter. These small contributions are ever pouring in and ever accumulating. This continual addition of dissolved mineral salts without any corresponding abstraction by evaporation has been going on ever since the surface of the earth has consisted of land and water.

An examination of the composition of other bodies of water, which, like the ocean, receive rivers or rivulets and have no other outlet than that afforded by evaporation, confirms this view. All of these are more or less saline, many of them more so than the ocean itself. On the great Table Land of Asia, "the roof of the world," there is a multitude of small lakes which receive the waters of the rivers and rivulets of that region and have no outlet to the ocean. On a map they appear like bags with a string attached, the bag being the lake and the string the river. All these lakes are saline, many of them excessively so, simply because they are ever receiving river-water of slight salinity and ever giving off vapor which has no salinity at all. There is no wash through these lakes as in the great American lakes or those of Constance, Geneva, etc.

The Sea of Aral and the Caspian are lakes without any other outlet than evaporation, and they are saline accordingly. The Dead Sea, which receives the Jordan at one end and a multitude of minor rivers and rivulets at its other end and sides, is a noted example of extreme salinity. It is, as everybody knows, a sea or lake of brine. The total area of land draining into the great ocean does not exceed one-fourth of its own area, while the Dead Sea receives the drainage and soluble matter of an area above twenty times greater than its own, and thus it fulfils the demand of the above-stated theory by having far greater salinity than has the great ocean.

According to this view the salinity of the ocean must be steadily though very slowly increasing, and there must be slowly proceeding a corresponding adaptation or evolution among its inhabitants, both animal and vegetable. The study of this subject and the effect which the increasing salinity of the past must have had upon the progressive modifications of organic life displayed by fossils is, I think, worthy of more attention than it has hitherto received from palæontologists.

## THE ENERGY-FUNCTION OF THE MAGNETIC CIRCUIT.

BY CHAS. P. STEINMETZ.

IN designing alternate-current electric motors, in October, 1890, I was confronted by the problem, to calculate the loss of energy caused by the reversals of magnetism in the iron of the motor-field. At that time very little was known on this phenomenon besides a few experimental data of Ewing. From these data mathematical analysis yielded the result that the loss of energy (by conversion into heat) during a complete cycle of magnetization is proportional to the 1.6 power of the intensity of magnetization, or magnetic induction,  $B$ ; that is, can be expressed by the formula:—

$$H = \eta B^{1.6}$$

where  $H$  is the loss of energy per magnetic cycle, and  $\eta$  a "co-efficient of hysteresis." This result was published in the *Electrical Engineer*, New York, December, 1890.

But it was not quite satisfactory, in so far as Ewing's determinations were made by the magnetometer method, with very slow cyclic variations of magnetism, and it was doubtful whether for very quick cycles, as they take place under the influence of an alternate-current of 100 or more complete periods per second, the same law holds, and especially the co-efficient of hysteresis,  $\eta$ , is the same.

A great number of tests, made during the year 1891, partly by the three-electrodynamometer method, partly by the use of the Eickemeyer differential magnetometer, and published in a paper read before the American Institute of Electrical Engineers, January, 1892, proved that up to over 200 complete magnetic cycles per second the loss of energy per cycle—by conversion into heat—is constant and independent of the number of cycles per second, following the law of the 1.6 power; while, when under the influence of the alternating magnetism, Foucault—or eddy—cur-

rents are induced in the iron, the energy-function of the magnetic circuit follows the more general law:—

$$H = \eta B^{1.6} + \epsilon NB^2,$$

where  $H$  gives the loss of energy per cycle and  $cm^3$ , in absolute units,  $N$  is the frequency, or number of cycles per second,  $\eta$  the co-efficient of molecular friction or hysteresis, and  $\epsilon$  a co-efficient of eddy currents.

Herefrom the loss of power per  $cm^3$  of iron, in Watts, is derived, as

$$W = NH \times 10^{-7} = \left( \eta NB^{1.6} + \epsilon N^2 B^2 \right) \times 10^{-7}$$

The next problem was now, to determine the energy-function of the magnetic circuit for cyclic variations of magnetism between any two limits,  $B_1$  and  $B_2$ , and to derive numerical values of the co-efficient of molecular friction,  $\eta$ , for different magnetic materials.

In the meantime, it had been found by Kennelly (Transactions of American Institute of Electrical Engineers, October, 1891) that Fröhlich's formula of magnetic induction —

$$B = \frac{H}{a + bH}$$

(where  $H$  is the magnetomotive force or field-intensity,  $a$  and  $b$  constants), which had been abandoned already as inexact, holds rigidly by a slight modification. Using not the whole magnetic induction,  $B$ , but the "metallic-magnetic induction,"  $L = B - H$ , where  $H$  is the field-intensity, we find, that for infinitely large field-intensities  $H$ , the metallic induction  $L$  approaches a finite limiting value  $L_\infty$ , and follows Fröhlich's formula:—

$$L = \frac{H}{a + \sigma H},$$

or, if we assume Ohm's Law for the magnetic circuit,

$$L = \frac{H}{\rho},$$

where  $\rho$  is the magnetic resistance, or reluctance, it is

$$\rho = a + \sigma H;$$

that is, the magnetic resistance is a linear function of the field-intensity.

On the hand of a large number of experimental determinations, made by the electro dynamometer and by the magnetometer method, comprising several thousands of readings, I was enabled to communicate to the American Institute of Electrical Engineers at the meeting of September, 1892, the results:—

1. The loss of energy by molecular-magnetic friction, per cycle and  $cm^3$ , for a cyclic variation of the magnetic induction between the limiting values  $L_1$  and  $L_2$ , is expressed by the function —

$$H = \eta \left( \frac{L_1 - L_2}{2} \right)^{1.6}$$

where  $L_1$  and  $L_2$  most likely have to represent the metallic induction  $L = B - H$ .

When Foucault — or eddy — currents are present in the iron, the energy-function of the magnetic circuit takes the more general form —

$$H = \eta \left( \frac{L_1 - L_2}{2} \right)^{1.6} + \epsilon N \left( \frac{B_1 - B_2}{2} \right)^2$$

where the first term gives the energy converted into heat per cycle and  $cm^3$  by true molecular friction, the last term the energy converted into heat by Foucault currents.

2. Beyond a minimum value of field-intensity,  $Hm$ , the metallic magnetic resistance follows the linear law:—

$$\rho = a + \sigma H.$$

3. Beyond this minimum value of field-intensity,  $Hm$ , all the essential properties of magnetic materials can be expressed by three constants:—

- $a$ , the co-efficient of magnetic hardness;
- $\sigma$ , " " " " saturation;
- $\eta$ , " " " " hysteresis;

or, instead of this, by the three constants:—

$L_\infty = \frac{1}{\sigma}$ , the value of absolute (metallic) magnetic saturation.

$H_0 = \frac{a}{\sigma}$ , the critical field-intensity, or that field intensity where

half-saturation,  $\frac{L_\infty}{2}$ , would be reached, if the linear law of magnetic resistance held already for this field-intensity  $H_0$ , and

$H_\infty = \eta L_\infty^{1.6}$ , the maximum value of hysteretic loss, by means of the formulas:—

The equations of magnetic resistance, or reluctance —

$$\rho = a + \sigma H = \frac{H_0 + H}{L_\infty}$$

(corresponding to Ohm's Law in the electric circuit), and the energy-function —

$$H = \eta \left( \frac{L_1 - L_2}{2} \right)^{1.6} = H_\infty \left( \frac{L_1 - L_2}{2 L_\infty} \right)^{1.6}$$

(corresponding to the energy-function of the electric circuit,  $W = e^2 R$ ).

4. These equations hold for all kinds of iron and steel, for nickel, cobalt, and magnetite, and most likely for the amalgams of iron, that is, for all magnetic materials.

5. In first approximation, the magnetic induction,  $B$ , and the magnetic hysteresis, or molecular friction,  $H$ , depend upon the magnetic field-intensity,  $H$ , by the law of probability of molecular distances.

6. Average values of magnetic constants are:—

For wrought-iron, soft cast-steel, and mild metal —

$$a = .38 \times 10^{-3} \quad \sigma = .055 \times 10^{-3} \quad \eta = .003 \quad (Hm = 9);$$

for cast-iron and low-permeability cast-steel —

$$a = 3 \times 10^{-3} \quad \sigma = .095 \times 10^{-3} \quad \eta = .013 \quad (Hm = 22);$$

for soft welded-steel and medium-hard cast-steel of high permeability —

$$a = 1.7 \times 10^{-3} \quad \sigma = .06 \times 10^{-3} \quad \eta = .02 \quad (Hm = 50);$$

for glass-hard steel —

$$a = 10 \times 10^{-3} \quad \sigma = .1 \times 10^{-3} \quad \eta = .07 \quad (Hm = 110).$$

October, 1892.

## THE APPARENT GROWTH OF GOLD.

BY RICHARD EAMES, JR., M.E.

OF the many myths prevalent regarding gold, the greatest one of all is its growth. Of course there are many interesting instances where ancient worked-out galleries in mines are slowly closing up by the incrustation process, so that space long ago excavated is being filled with an accumulation resulting from the percolation of water through the adjacent wall-rock. This water has in chemical combination such minerals as iron, copper, sulphur, and the precious metals, which are deposited in the open crevice, making for a second time a mineralized body which will show by analysis the above named and many more minerals. In fact, I have had this actual experience resultant on the examination of an old gold mine in Honduras, Central America, that had been worked some time prior to any history we have of that country. This circumstance gave to the natives the idea that gold grew, and they so expressed themselves; while it seemed in the case of one individual a transmutation idea had permeated his head, for he explained that the green carbonate of copper was undergoing a change into silver, while the silver in turn would develop into gold.

In India I found a caste of mining people who believed that gold grew in the bottom of the large lakes situated in that country. They expressed no practical reason other than fairy-tale superstitions. And even in this country there are converts to the idea. I was much amused and interested some years ago to hear an intelligent acquaintance maintain in strong and not altogether religious terms that "the stuff grew and he knew it." His experience was based on the fact that in a certain pile of tailings, resulting from the milling of heavy sulphuretted gold-ores, he had treated at one time several tons with no result. Again, in

three years time he discovered by a pan-test that the same pile had gold in it, whereupon he treated several tons with the approximate result of one dollar per ton. After this last treatment he declared the tailings were barren of gold so far as he could detect by the mechanical means at hand. In order to convince him I selected an average sample, which on assay gave 2 pennyweights, 20 grains gold per 2,000-pound ton. This was considered not worth the working. But my friend maintained that the gold would grow again in two or three years.

True to his word, in two years he was at the pile again, and by his crude but sure method was saving one dollar per ton from the ore that would yield *nil* by his methods two years before. Again, I took samples for assay and was somewhat surprised to find the value had increased just 50 per cent, as the result of my determination was 5 pennyweights, 16 grains per ton. On investigation, I found the sulphurets to be of a character readily decomposed by the elements; in fact to such an extent that, as I afterwards calculated, over one-half of the pile must have been decomposed or washed away, so that with the decomposition a certain rapid concentration was maintained by the action of heavy rains, and the natural advantage this particular ground offered causing the gold to remain behind while the oxides were carried away in suspension by the water. My explanation has failed to convince my friend of the pick and shovel. As the gold in the tailings has become about exhausted, his last attempt to make pay was a failure. He remains strong in the conviction that a few years will grow it again.

Gold Hill, North Carolina, October, 1892.

#### CURRENT NOTES ON ANTHROPOLOGY.—XVIII.

[Edited by D. G. Brinton, M.D., LL.D.]

##### The Early Age of Metals in Europe.

As has been previously remarked in these Notes, there is a growing tendency in European archæology to rate the civilization of Europe at the dawn of the historic period decidedly higher than has been heretofore supposed, and to regard it more and more of indigenous development. Those old theories which attributed pretty much all that deserved the name of culture to Asiatic or Egyptian sources are diminishing in favor.

An instance of this is seen in an article by M. Salomon Reinach in *L'Anthropologie*, 1892, No. 3, in which he discusses with his accustomed wealth of erudition the derivation of the name "Cassiterides," and with it the origin of tin and bronze in western Europe. He claims that this name is of Celtic origin, and means "Remote," or something of that kind. It was applied by the Gauls to the portion of western England whence came the tin. This conclusion proves several points, if once accepted. As Homer talks of the Cassiterides, it shows that before his time the tribes of western France spoke Celtic; that they worked in and exported metals; and it gives room to inquire whether one of the centres of the discovery of bronze was not in western Europe.

Other archæologists of ability, such as Franz von Pulszky, in the *Archiv für Anthropologie*, Bd., XX., have called attention to the fact that the specific civilization of the Celts was higher than is generally recognized. Their heavy iron swords, for striking, not thrusting, their ornamentation, derived from the circle and the triangle, and their use of torques, wound metal neck-rings, reveal positive ethnic art-capacity. Their presence in Hungary is well marked by such remains in the tombs of an early epoch.

##### Figurines of the Stone Age.

The glyptic art goes back far into the stone age, far even into the old or rough stone age. In the *Antiqua* for 1887-1890, R. Ferrer has discussed and depicted the earliest human statuettes from the European bronze and stone ages. The oldest always represent the individual naked, and the parts of sex very prominent. This is also the case with the Phœnician bronze figurines from Ellora, in Portugal, while those from the north are clothed. Last December there were found some interesting remains near Brunn, Germany, at a depth of four and a half meters, amid bones of the mammoth, rhinoceros, and reindeer. They

were a human skull, and adjacent to it a human figurine 20 centimeters high, carved from the tooth of a mammoth, and bored through, evidently for the purpose of suspension. The figure is naked and prominently masculine, though the mammæ are clearly represented.

The skull presented an index of 65.68, and was therefore singularly dolichocephalic; its estimated cubical capacity was 1,350 cubic centimeters; it was not prognathic, but the frontal sinuses were very prominent, and the glabella also, thus presenting an inferior character.

When the head of the figurine is regarded in profile, it presents this peculiar appearance of prominence in the glabellar region, thus showing that it was carved to imitate the then prevailing type of humanity.

These and other interesting facts about this noteworthy find are given in the Proceedings of the Niederrheinische Gesellschaft, 1892, by Professor Schaaffhausen of Bonn, who adds an engraving of the skull. Like all his articles, this one is prepared with the most satisfactory care.

##### The Study of Hair.

The study of the hair on man offers a most extensive field of inquiry, and one which presents many unsolved problems of the first order of importance. Some of these are discussed by F. Lapille in *Le Naturaliste* and by Dr. Bartels in the *Zeitschrift für Ethnologie* of recent dates.

Why man as a species should present the amount and kind of hair that he does is variously explained, and the differences between the varieties of the human race are so great in this respect that, as is well known, one of the most popular subdivisions of the species is founded upon it. Most mammals have more hair than man, but some less, as the Cetaceæ and the Sirenidæ. The anthropoid apes have, as a rule, much hair where man has little, as in the arm-pits and around the sex-organs. In some localities, as the ears and nose, the hairs are clearly protective organs, while around the genitals they appear to be merely ornamental. In monkeys, the females are bearded, but such examples are rare in the human species. Bearded women, however, are not otherwise masculine, but have the sentiments and the capacity for motherhood. Bartels describes a very hairy Gypsy girl, only seventeen years old, but already the mother of three children. With her the hairiness was from a naevus pigmentosus of extraordinary extent; and why these naevi should develop hairs is worth inquiry. Man has the longest hair of any animal, and why he lost it over most of his body is the subject of much curious speculation. The loss led him to the inventions of painting and tattooing his body, of covering it with clay or clothes, to depilation, to the sense of modesty, and to many other unexpected results. The history of hair in man is thus an extraordinary one for the evolution of the species.

##### On Quarry-Rejects.

For two or three years past there has been in the air — I mean the air which archæologists breathe — a low but menacing sound, threatening some dear theories and tall structures, built, if not on sand, at least on gravels offering a scarcely more secure foundation.

These menaces bear more directly on what is classically known as the "old stone age," that of chipped implements, and particularly on that period of it which is alleged to be characterized by very rude — and which are therefore supposed to be very old — types. The new views come from a study of the aboriginal quarries, the sites where the ancient tribes collected the materials which later and at other localities they worked up into finely chipped or polished implements. This part of the work they did not perform at the quarry; and pieces which after a few test-blows by their skilled hands they saw could not be utilized at all, or only at the cost of considerable labor, they threw aside and left on the spot. These are "quarry-rejects," and after you have handled and studied several hundreds of them you can always see why they were thrown away; you can recognize, as did the aboriginal artist, why they would prove worthless or troublesome in further working.



Now the alarming discovery has been made that a great many of what we have heretofore called "palæolithic implements" display with fatal clearness the peculiar earmarks of these "quarry-rejects," hinting, therefore, that they never were real implements at all. What is worse, like the rejects, they show no signs of use, and clearly never could have been employed as implements, and consequently do not in any way illustrate the industry of the chippers, no matter of what age they are. If found in gravels, the gravel-bed was the quarry, and they the refuse. It has even been hinted that the famed gravels of the Somme and the "palæolithic floors" of the Thames and the "Trenton gravels" of our own land, may have to lose their laurels in the light of this discovery.

#### The Builders of the Southern Mounds.

Those who have looked at the archæological collections of the Smithsonian with any attention, cannot fail to remember the extraordinary specimens of copper work from the Etowah valley mounds, in northern Georgia. The figures they delineate have an unquestionable family resemblance with those inscribed on shells obtained on the lower Mississippi, so accurately presented in Mr. Holmes's essay in the Report of the Bureau of Ethnology, 1880-81. Both present curious analogies to Mexican and Maya art, and I have been almost constrained to believe in a connection, either ethnic or commercial, between these peoples.

Dr. Eduard Seler, however, who is a most competent authority on these questions, expresses a different opinion in a recent article in *Globus*, Bd. LXII., No. 11. He analyzes with care the mode of wearing the hair, the headdress, the clothing, and the weapons of the figures, and shows that in several of these points they correspond with the descriptions of the early voyagers of the natives they found in these localities. He also compares the same features with similar relics from ancient mounds in the Ohio valley. The conclusions he reaches are, that the builders of the Etowah mounds and the artists of the inscribed shells were probably related to the builders of the Ohio mounds; that they were not the direct ancestors of the tribes found in Georgia at the discovery; that there is not sufficient reason to suppose connection with Mexico or Yucatan; that probably the mound-building and copper-working tribes were destroyed or driven to the remote sea-coast by invasions from the north and west at a period not very remote from that of the discovery of the continent.

#### LETTERS TO THE EDITOR.

**\*\* Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.**

**On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.**

**The editor will be glad to publish any queries consonant with the character of the journal.**

#### "Ancient Mexican Heraldry."

PERMIT me to place before the readers of *Science* an interesting fact kindly communicated to me on Oct. 17 by the Rev John Woodward, LL.D., F.S.A., the learned author of "A Treatise on Heraldry, British and Foreign," referred to in my article on "Ancient Mexican Heraldry" in *Science*, No. 503, Sept 23. That gentleman has just informed me "that the late Mr. Ellis, in his book on 'The Antiquities of Heraldry,' 1869, made reference to the Armory of the Ancient Mexicans and gave a plate (iv.) of the symbols from the works of Lord Kingsborough and Mr. Stephens." "There is not, however," my courteous correspondent adds, "any evidence that the use of such devices was hereditary; they were, so far as we know, merely personal distinctions." This agrees with the general conclusions, based on special investigations, of Mrs. Zelia Nuttall, who was doubtless, like myself, unaware of the publication of Mr. William Smith-Ellis's views on Mexican heraldic emblems nearly a quarter of a century ago. I endeavored in vain to consult a copy of his work when studying the subject of my communication to *Science*; and, as other students may experience the same difficulty, I will reproduce the Rev. J. Woodward's observations respecting ancient heraldry on page 26 of the Introduction to Vol. I. of his recent Treatise on Heraldry in general.

"Mr. W. G. (sic) Ellis, in his 'Antiquities' of Heraldry," has collected a mass of interesting matter relating to what he calls the heraldry of ancient times and of all nations of the world, and he certainly succeeds in showing to how great an extent pictorial symbols, which had originally a meaning, have been in use among all nations of mankind, civilized and savage. His plates are curious as showing the occasional occurrence among these manifold devices of some resembling modern figures of blazon. The crescent, the mullet, the lozenge, the quatrefoil, and the fleur de lis are traced by him to counterparts existing among Egyptian, Chinese, Indian, and Japanese emblems; and among the figures on Etruscan vases he shows us what, in heraldic language, would be called a bull's head caboshed and a not unheraldic-looking demi-boar."

Furthermore, it is noteworthy that the Rev. John Woodward considers "there is some reason to believe that the use of the hereditary badge must have long preceded hereditary heraldry" (p. 589). Additional instances of their use as military distinctions rewarding the capture of prisoners in European warfare may be cited from his interesting work. Two fleur de lis with other "augmentations" were granted to Sir John Clerke because he took captive Louis Duke de Longueville on the field of Therouenne, better known as the battle of the Spurs, and fought a month before the eventful fight of Flodden. A still more interesting case is that of the "Crampet," or metal termination of the scabbard of the sword, sometimes borne as a device. One was assigned to Sir Roger La Warr about the same time as the buckle was granted to Sir John Pelham in recognition of his aid in the capture of King John of France. It is somewhat remarkable that the descendants of these gallant knights, who fought side by side at Poitiers, still hold lands and earldoms in the same County of Sussex. Some members of the ancient house of De la Warr passed over to America in the fifteenth and sixteenth centuries, hence the more familiar name of Delaware.

AGNES CRANE.

Brighton, England, Oct. 21.

#### Reticulated Protoplasm of *Pelomyxa*.

SINCE the publication of Dr. Stokes's article myself and colleague, Mr. W. F. Pentland, have had several opportunities of examining *Pelomyxa Palustris*. The difficulties of the investigation are so great that at his suggestion I tabulate the methods and their results.

1. Examination of unstained preparations (50 slides). Utterly useless as far as the detection of reticulation is concerned. 2. After treatment with osmic acid, usually 1 per cent solution, one organism was found destitute to a great extent of ingested material. An  $\frac{1}{2}$  Powell and Leland water-immersion and Zeiss 12 compensating ocular failed even with critical light, with an immersion condenser, and ammonia sulphate of copper solution as modifier, to detect the slightest trace of reticulation. 3. In the same preparation we found some Amœbæ resembling Proteus. The coarseness of the enclosure in these specimens we found would lead a neophyte astray, as it frequently resembled reticulation. 4. Determined to leave no stone unturned, we tried the usual aniline and carmine dyes, with no result. 5. One-half per cent solution of chloride of gold (no osmic acid) was tried on over twelve slides. I must certainly confess I glimpsed reticulation in two specimens, but owing to the protracted investigation was compelled to desist.

So far with regard to amœbic organisms. It is in the field of pathology that reticulation of protoplasm is most frequently observed, as far as my experience goes. The more rapid the morbid process the greater certainty of reticulation. Fifteen years ago I was working with my lamented colleague, Dr. Booke of Dublin, on the effects of bichloride of mercury on blood corpuscles; but fortunately we did not follow out Dr. Klein's suggestions to the letter, as we found epithelium cells beautifully reticulated as described by Dr. Stokes. We found the nucleus in the blood discs, but, as usual in scientific investigations, forgot to look for reticulation. However, on a future occasion the late Sir Robert Macdonald submitted to us for examination a portion of tumor of then unknown nature. The portion was placed in Muller's

fluid when sent, and that evening sections were cut and mounted after short treatment with picro-carmine. Without examination two slides were sent to Sir Robert (we were busy on small-pox), who returned them with the remark, "Only muscular fibre."

Dr. Bookey looked at me and I gazed upon him, we then subjected the slide to examination with  $\frac{1}{16}$  water-immersion Powell and Leland and No. 2 eye-piece, all apparatus being Powell and Leland. I have seen reticulation since, but in a tumor purely epitheliomatous; it was simply wonderful. The cells were perfectly differentiated, and the reticulation was so regular that we at once forgave Sir Robert for his hasty conclusion.

We hope to continue our investigations on amoeboid organisms; but, as the process is so long, my colleague persuaded me to send you these remarks.

A. COWLEY MALLEY.

Munslow, England.

### The Fundamental Hypotheses of Abstract Dynamics.

I HAVE been prevented from making earlier reference to Mr. Dixon's letter in *Science* of Sept. 9, p. 149, criticising my address on the above topic, *Science*, Aug. 5, p. 71. The letter was especially interesting to me as I had not seen his paper, "On the Logical Foundations of Applied Mathematical Sciences," communicated to the Mathematical Society of London some few months ago.

Mr. Dixon, taking the relativity of direction into account, seems to me to have proved that the Laws of Motion may be regarded as forming a definition of force. My argument to show that if they be so regarded, they are not in general consistent with one another, involved the specification of accelerations by reference to a single point, and thus assumed the possibility of determining directions absolutely. While valid, therefore, as against the writers to whom I referred, who make the same assumption, it has not the more general validity which I supposed.

That I have regarded force as a non-relative conception, while Mr. Dixon has thus shown that it may be regarded as relative, would seem at first sight to place us in antagonism. It does not, however; for I have merely discussed certain points in connection with the laws of motion, employing the ordinary conception of force, and making no inquiry as to the assumptions involved in it, while Mr. Dixon proves that this conception must involve certain assumptions, and seeks to determine what they are.

Mr. Dixon points out that it is the law of the conservation of mechanical energy only which is deducible from the assumption that stresses are functions of the distance between the particles on which they act, and that this law would not include the general law of the conservation of energy until all energy was shown to be mechanical. That is quite true; but it does not seem to affect my contention, that, since we are now so sure of the conservation of all forms of energy that the law of the conservation of mechanical energy is frequently assumed as itself axiomatic, the laws of motion, if they are to be retained as dynamical axioms, should be supplemented in such a way that this law would be deducible from them. Nor does the fact that the law of the conservation of energy is usually expressed at present in a form which is probably temporary seem to me to make this any the less desirable. The conception of potential energy may lose its utility as we gain clearer insight into dynamical phenomena. When that time comes we may have to modify our fundamental hypotheses to suit the clearer views which will have been gained; but in the meantime it seems none the less desirable that we should have axioms sufficient for the deduction of the law of conservation in its present form.

There is, as Mr. Dixon supposes, an omission in the sentence of my paper which he found unintelligible. If commas be inserted after the words *sum* and *masses*, it will be found to state that, if  $m_1$  and  $m_2$  be the masses of two particles, and  $a$  the relative acceleration produced by a stress between them, this stress may be shown to be proportional to

$$a m_1 m_2 \div (m_1 + m_2).$$

It follows that, if one of the particles be of infinite mass, the stress is proportional to the mass of the other multiplied by the relative acceleration. When I conclude from this that "if, in

applying the second law of motion, a particle of infinite mass be chosen as point of reference, all the forces acting on a system of particles, may be regarded as exerted upon them by the particle of infinite mass," these forces are supposed to be exerted in accordance with the third law of motion, which asserts action and reaction to be equal and opposite, but not to be in the line joining the particles acting on one another. I do not myself regard this fiction as of any importance. I mentioned it in passing because I wished to refer subsequently to Newcomb's assertion that the law of the conservation of energy assumes it.

Mr. Dixon considers it inconvenient to include in one law of stress two statements resting on such very different evidence as that forces may be considered to be attractions or repulsions and that their magnitudes depend solely on the distances between the particles on which they act. I need hardly say, however, that I see no objection to enunciating the two statements in separate sentences. For educational purposes, indeed, it would certainly be well to enunciate what I have called the law of stress, piecemeal, as is invariably done in the case of what I have called the law of force.

J. G. MACGREGOR.

Dalhousie College, Halifax, N. S., Oct. 4.

### The Libyan Alphabet.

I GLADLY accept Dr. Brinton's offer (*Science*, Sept. 30); only, if his object is truth rather than the scoring of a point, he will place in the editor's hands, not the *Grammaire tamacheek*, which would be useless for the purpose, but the *Grammaire kabyle*, which alone contains the full forms of the three Berber alphabets, but which Dr. Brinton appears never to have seen. Even the *Grammaire tamacheek*, now that he has got hold of it, he seems incapable of understanding. The other day he mistook diacritical marks for accents, and now he tells us that Hanoteau connects the Libyan and Semitic systems "solely" because both are read from right to left, even charging me with disingenuousness for suppressing this fact. The charge might stand, had I made the assertion, which is as wide of the mark as is Dr. Brinton's appeal to Hanoteau, on the question of accent. The very Berber name *asekkil* (pl. *isekkilen*) of the letters is equated by Hanoteau (p. 5) with the Arabic *shakl* and the Hebrew *sakal*, "forme, figure, dont les Grecs ont fait *συγλαι*," hence the French *sigle*. I am not defending these equations, but merely give them to show how ignorant Dr. Brinton still is of the contents of the *Grammaire tamacheek*, which he had the temerity to insinuate I had never seen (*Science*, Aug. 19). May I ask Dr. Brinton who are the "French scholars" that regard the initial *t* as radical in the word *tifinar*, and that accent the word differently from Barth, for this also appears to be again insinuated? The recent death of M. E. Renan reminds me that that illustrious "French scholar" is also arrayed against Dr. Brinton, holding that the Punic origin of the Libyan alphabet is an established fact (*Histoire des langues semitiques*, 2d ed., p. 194. *et seq.*). Dr. Brinton is to be envied his possession of "plenty of documents in *tifinar*." Such documents are excessively rare in Europe, and even amongst the Tuaregs themselves, who, apart from rock inscriptions, have never made any extensive use of this old and defective script. Considering the weakness of his position, Dr. Brinton shows as much want of tact as of bad taste in charging his opponent with lack of candor.

A. H. KEANE.

79 Broadhurst Gardens, South Hampstead, N. W.

### Is There a Sense of Direction?

IN his article on the "Sense of Direction," in *Science* of Oct. 7, Dr. Work says, "It is very well known that an unguided horse returning to familiar haunts will do so over the same route by which he left them, rather than in a direct line by sense of direction." An incident which came under my observation some six years ago directly contradicts this theory. My father had purchased a very intelligent mare about a month before, and on this occasion I hitched her single to a carriage, and drove to a town about fourteen miles distant. As the direction was almost due north-west, the road ran alternately west and north, there being about eight corners to turn. Although the mare might have been

through the same region before, it is pretty certain that she had never travelled just the same road. Coming back I gave her her head, and she made every turn so as to keep the same road as on the going trip, with one exception. In that case she made a short-cut by a diagonal road across a quarter-section, striking the regular road a mile further on, and saving about a quarter of a mile. In going up I should have taken the same route, had I not had some business which required me to go the longer way. At the point where this road turned off, it led toward a hill which concealed its junction with the regular road. I certainly did nothing to guide the mare, and was astonished to see her take the short-cut.

As Dr. Work has left considerable room for "accident," he may be able to dispose of this circumstance in that way, though I can scarcely accept such an explanation.

Brookings, S.D.

J. M. ALDRICH.

THAT the sense of direction is feeble, if indeed present, in civilized man cannot be denied. I have had some experiences which lead me to suspect that it may be obsolescent rather than quite obsolete. It has frequently occurred that in coming into a strange town or city at night, when compelled to abandon all conscious effort to keep my direction, I have found that in some way I had not lost the points of the compass. These may have been happy accidents, but they may have been cases of unconscious orientation.

Again, upon visiting a cave of considerable dimensions, I purposely refrained from any conscious effort in keeping the points of the compass, with the same result as in the preceding cases.

To the foregoing I have added some inquiries, and a few observations upon others, and feel that there is some ground for thinking that there may be a feeble sense of direction still left to us, though so feeble as to be easily overborne by suggestion from the other senses.

University of Nebraska, Lincoln.

CHARLES E. BESSEY.

#### On Biological Nomenclature.

I AM glad to learn, from Dr. Coues's letter in *Science*, that the code of rules promulgated by the American Ornithological Union a few years ago has been rigidly enforced in that branch of biology, and has been found to work admirably in practice. I studied these rules at their appearance with much interest and attention, and have since, so far as possible, endeavored to adhere to them in my own writings, with one exception — that concerning the erection and definition of genera. As I see that the botanists are disposed to accept this same rule, I shall be glad if a wider discussion may be called out before it becomes established. I refer to canon xlii., which recognizes the validity of generic names unaccompanied by definition, if described species are pointed out as types.

Among ornithologists, and perhaps among botanists, such a rule may not be productive of as much confusion and annoyance as is sure to be the case among entomologists. Generic characters are not, and should not be, included in specific descriptions; how then is it possible for the remote student to learn what *nomina nuda* mean, when it is impossible for him to study the types? He who studies only his own immediate fauna or flora, without a knowledge of the allied forms throughout the world, can never be very successful as a systematist, and, if we are to rely upon types, what is the good of a scientific nomenclature? Furthermore, in such a science as entomology, where there is still a tendency to look upon the manufacture of species and genera as the *ultimum bonum* of the systematist, the mere possibility of such a rule obtaining currency must have a tendency to foster superficiality, incompetence, and ignorance. While I do not agree wholly with those who look upon the genus as an abstract thing, over and above types, I do protest strongly against the acceptance of a rule that will relieve the namer from the necessity of knowing anything about the things he names.

The fear of evil results is not a groundless one. Some years ago an Italian writer, with an assurance as boundless as his ignorance, brought forth a new "system" of dipterological classification, with hundreds of new names. Not the slightest attention has ever been paid to his "system;" but, with this rule in force,

one would be bound to torture himself in trying to unravel its vagaries. The careless writer should have no such rule, the careful writer needs none.

S. W. WILLISTON.

University of Kansas, Oct. 18.

#### Solid Glycerine.

CAN you inform me, through your magazine, by what chemical, or by what process, glycerine may be solidified, retaining its transparency? Can any reader answer?

C. C. SMITH.

New York, Oct. 31.

#### BOOK-REVIEWS.

*Fourteenth Annual Report of the State Board of Health of the State of Connecticut for the Year Ending November 30, 1891.*  
New Haven, 1892.

THIS report presents fresh evidence that the work undertaken by the various State boards of health is steadily increasing both in scope and in value. This encouraging condition of things has been brought about largely by the adherence of several States to the policy of employing competent expert service. The authorities of these States consider that scientific problems can be successfully attacked only by the most advanced scientific methods, and have in consequence availed themselves of the aid of highly trained chemists, biologists and engineers. A great impetus has been given in this way to the best kind of sanitary work.

The Connecticut report contains, besides the usual reports from local boards of health and the annual statistics of births and deaths, several special features of more than ordinary importance and interest. Dr. H. E. Smith presents a special report upon "The Origin of Certain Cases of Typhoid Fever from Money Island." Twenty-one cases of typhoid, one of which proved fatal, were traced to the contaminated water used at a hotel on Money Island. From . . . facts concerning the sources of the water used, it appears that during the period August 11 to 14, at which time all of those subsequently taken ill were at the inn together, the drinking-water was obtained from the billiard-hall cistern." Dr. Smith shows further that abundant opportunity existed for the infection of this particular cistern water, and adduces convincing evidence that the water was actually infected by a case of "walking typhoid," and that the water thus infected spread the disease.

Dr. L. S. DeForest, in his article upon "Tuberculosis as a Local and Contagious Disease in New Haven" discusses the interesting question of infected dwellings. Dr. DeForest found from the data of 1876-1890 three principal districts of concentration of tuberculosis in New Haven. From a detailed study of house cases he arrived at the conclusion that houses sometimes became true foci of infection. "We think that the accompanying maps and tables go far to show that consumption is endemic in certain parts of the city; that in these parts there are many houses in which it is distinctly dangerous to live." The value of Dr. DeForest's interesting paper would be considerably enhanced by the addition of exact references to the writings of Flick, Cornet, and the other workers in this same field.

The report of the "Examination of Certain Connecticut Water Supplies," by Drs. Samuel W. Williston, Herbert E. Smith, and Thomas G. Lee, covers some two hundred pages and is illustrated with a number of well-arranged charts showing the monthly variations of the analyses. In some respects the report merely confirms the previous work of the Massachusetts State Board of Health, but in other respects it improves upon and extends the latter. Fifteen different water supplies were selected for study, and monthly examinations were made of most of these during a period of twenty-three months.

The special report on the chemical examinations is by Dr. Smith, who in his methods follows closely the chemists of the Massachusetts staff. He, however, expresses his results in milligrams per litre rather than in parts per hundred thousand, and makes a few other minor clerical changes. The limited resources at his command did not permit him to take up carefully the interesting and important question of "normal chlorine;" but his chlorine determinations, so far as they go, support the work of

Professor Drown. It is much to be desired that extended and accurate determinations of normal chlorine should be made in all parts of the country. The importance to the water-analyst of such knowledge has been clearly demonstrated by Professor Drown, and there seems no reason why boards of health should fail to take advantage of so useful a key to the purity of a water. Regarding the disagreeable odors and tastes which often arise in waters Dr. Smith coincides with Professor Drown in concluding that these phenomena are rarely due to putrefactive change, but are "the specific odors of certain organisms."

The microscopical examinations were conducted under the charge of Dr. Williston, who used in his work the method devised by Professor Sedgwick, modifying it, however, in more or less important details. Valuable facts are here presented concerning the occurrence and relative abundance of the different groups of small aquatic organisms. A very interesting description is given of the trouble caused in the Meriden reservoir by the Volvox-like Uroglena. An epidemic of Uroglena has since broken out in several Massachusetts water-supplies, and new light upon the history of this obscure and troublesome organism may soon be hoped for. "In conclusion, it may be interesting to observe that on the average about seven thousand plants and animals, aside from the bacteria, are swallowed with every glassful of Connecticut reservoir water that is drank!"

Dr. Lee reports upon the microscopical examination for 1890-91 and also upon the bacteriological examination for the two years covered by the report. The bacterial examination of the reservoir waters reveals some interesting facts. "The periods of greatest number [of bacteria] were principally during the fall months, while the smaller numbers were largely during the summer months." There is a decrease in the numbers in both February and March, the months of melting snow. It is evident that extended and systematic bacterial analyses of this character are still greatly needed, and it is to be hoped not only that Connecticut will enlarge this branch of the work, but that the Massa-

chusetts authorities will find it possible to make an early beginning in this same field.

The specific bacteria commonly found in Connecticut waters were studied by Dr. Charles J. Foote, who describes in this report sixteen forms. The species are not named, and the author says "No attempt at identification has . . . been made, since the data in most cases seemed insufficient to warrant it." This, we think, is a fact to be regretted. The species "XXI.," for example, might perhaps have turned out to be *Bacillus janthinus* if a few more facts regarding it had been obtained. Attempts to identify, however unsatisfactory in themselves, lead to the accumulation of new data and result in more adequate descriptions. The growth of the different species in gelatin is described with more than usual discrimination, and with the use of several apt comparisons.

A valuable study of the Connecticut River by Dr. Smith concludes the report.

*The Hygienic Treatment of Consumption.* By M. L. HOLBROOK, M.D. New York, M. L. Holbrook & Co.

By far the larger part of this book is devoted to the discussion of the prevention and treatment of consumption in its earlier stages. Under this head are given the author's views upon suitable exercises for persons of consumptive tendencies, upon clothing, climate, baths, and other familiar topics. These subjects are in the main handled with common sense and with perhaps more than the usual freshness of illustration.

The author calls attention to the dangerous weakening of the constitution arising from an incessant course of "colds," and gives some sensible practical suggestions for individual hygiene. The connection of colds with indigestion is clearly recognized. "Overloading the stomach with indigestible food is a frequent cause of colds. It is far better to wait a little before eating if one is very tired, or to eat slowly and moderately, so as to give the stomach time to regain its power" (p. 44).

#### CALENDAR OF SOCIETIES.

##### Society of Natural History, Boston.

Nov. 2.—George L. Goodale, Certain Aspects of the Vegetation of New Zealand.

##### Philosophical Society, Washington.

Oct. 29.—A. W. Harris, Agricultural Experiment Station Exhibit by the Department of Agriculture at the World's Fair; C. D. Walcott, Notes on the Lower Paleozoic Rocks Between the Susquehanna and Potomac Rivers; T. Russell, Relation of Rain-Fall to River Rise; W. Harkness, Flexure of Telescope Tubes.

#### Publications Received at Editor's Office.

- ACLAND, SIR HENRY W. The Inaugural Robert Boyle Lecture. Oxford, Henry Frowde. 12°. Paper. 45 p.
- ALLEN, HARRISON. On a New Sub-family of Phyllostome Bats. Washington, Government. 8°. Paper. 7 p.
- BAILEY, M. A. American Mental Arithmetic. New York, American Book Co. 16°. 160 p. 35 cts.
- BAKER & Co. Data Concerning Platinum, etc. Newark, Baker & Co. 11 p.
- DOUGHTY, FRANCIS F. Evidences of Man in the Drift. New York, Privately Printed. 8°. Paper. 18 p.
- HURST, J. T. Tables and Memoranda for Engineers. 11th ed. New York, Spon & Chamberlain. 182 p. 50 cts.
- JAMES, BUSHROD W. ALASKANA. Philadelphia, Porter & Coates. 12°. 386 p.
- LUBBOCK, SIR JOHN. The Beauties of Nature. New York, Macmillan & Co. 12°. 443 p. \$1.50.
- NADAILLAC, MARQUIS DE. Manners and Monuments of Prehistoric Peoples. Trans. by Nancy Bell. New York, G. P. Putnam's Sons. 8°. 424 p. ill. \$3.
- PEDDIE, WILLIAM. A Manual of Physics. New York, G. P. Putnam's Sons. 12°. 513 p. ill. \$2.50.
- SIMPSON, CHARLES T. Notes on the Unionidae of Florida and the Southeastern States. Washington, Government. 8°. Paper. 31 p. ill.
- TRUE, FREDERICK W. Annotated Catalogue of Mammals Collected by W. L. Abbott. Washington, Government. 8°. Paper. 35 p. ill.

#### Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

For Exchange—"The Birds of Kansas,"—Goss, for Gray's Anatomy, or Medical Dictionary. Must be in good condition. Address, J. H. SIMPSON, Buchtel College, Akron, Ohio.

For Sale or Exchange.—The subscriber would like to receive cash offers, or an offer in exchange for the earlier volumes of Poggendorf's *Annalen* and the later volumes of Silliman's *Journal*, upon the following list: Cheen—*Manuel de Conchyliologie*. 2 vols. Nearly 5,000 figures, some hand-colored. Paper. Paris, 1859. Edwards.—*Butterflies of N. A.* 2 vols. Plates hand-colored. Vol. I., half calf. Vol. II. in parts. Leyman, Agassiz, Hagen.—*Ills. Cat. Mus. Comp. Zool. at Harvard*. No. I. Ophiuridae. No. II. Acalephae. No. III. Astacidae. All bound in one volume. American Naturalist. Vols. I.-VII. Cloth. Silliman's *Am. Jour. of Science and Arts*. Third Series. Vols. I.-X. Cloth. Binney.—*Terrestrial Mollusks of N. A.* Colored plates. 4 vols. Stretch.—*Zygaenidae and Bombycidae of N. A.* Colored plates. Also a considerable library of monographs, reports, and scientific books, and a large number of duplicates of fossils, minerals and shells. E. A. STRONG, Ypsilanti, Mich., Sept., 1892.

Exchange.—I have the finest shells of Anodonta corpulenta, C'p'r, and Suborbiculata, Say, in the world. Will exchange for fresh water, land, and marine shells, in large or small quantities. Have also for exchange 50 varieties of fresh water and land shells from Spoon River, Ill. DR. W. S. STRODE, Lewistown, Ill.

#### Reading Matter Notices.

Ripans Tabules: best liver tonic.  
Ripans Tabules cure jaundice.

#### Wants.

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We question if every one would regard rowing in quite as favorable light as does our author. Paddling in a canoe, which is in many respects a much superior exercise, is unmentioned. Lawn-tennis, also, which is one of the best tools of recreative hygiene, does not find a place in our author's discussion. Horse-back riding gets its full due as an admirable form of combined diversion and exercise. Throughout this portion of the book the value of good digestion as an aid to good nutrition is justly insisted upon; and exercise is conservatively regarded as simply a means to the welfare of the whole organism and not as an end in itself.

It seems to us that the author does not emphasize as much as is desirable the means of prevention advocated by many professional bacteriologists. The destruction of tuberculous sputum, if any sort of concerted action can be obtained, will go far towards diminishing the chances of infection. The herding together of the consumptives at "health resorts," and the journeys thither in cars infected by their tuberculous predecessors are raising many serious problems. It will seem to many readers that this side of our author's subject receives inadequate attention.

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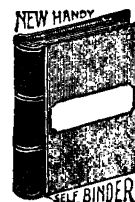
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